2. *On the Transportal of Erratic Boulders from a lower to a higher level.* By C. DARWIN, Esq., F.R.S., F.G.S.

IT will, I think, be generally admitted that the most valid objection which has been advanced against the theory of the transportal of erratic boulders by floating ice, lies in the fact of their having not unfrequently been carried from a lower to a higher level. Mr. Hopkins*, indeed, referring to certain boulders of a peculiar conglomerate described by Prof. Phillips, considers this fact as affording an absolute proof of the diluvial theory, since, he adds, "it is evident that no floating ice could possibly transport a boulder from the depths of the vale of Eden over the heights of Stainmoor." Prof. Hitchcock has several times alluded to similar cases in North America as offering a very great difficulty.

The first instance recorded, as far as I know, of the transportal of boulders from a lower to a higher level, is by Prof. Phillips†, who in 1829 described numerous large blocks of grauwacke not far from Kirby Lonsdale, scattered over the mountain limestone from a height of 50 to 100 feet above the parent rock, which lies immediately beneath. He adds, "Further on, to an elevation of 150 feet, the blocks are still numerous, and they may be seen, by ascending one ledge after another, almost to the top of the Fell, 500 feet above their original position. They appear," he continues, "to have been driven up at a particular place by a current towards the north, and afterwards carried along the surface of the limestone in a narrow track toward the summit of the Fell." The conglomerate alluded to by Mr. Hopkins has been transported from the bottom of the valley of the Eden, where the rock lies *in situ* at the height of 500 feet above the level of the sea, to and over the pass of Stainmoor at the height of 1400 feet‡: therefore the boulders now lie 900 feet above their original position. In 1838 I observed many boulders of granite strewed on Ben Erin on the western side of Glen Roy§, up to the height of 2200 feet above the sea; the granite resembled in character that seen *in situ* at the head of the Spey, and which, in Macculloch's Geological Map, is likewise the nearest district of granite: *if, as I believe, the boulders came from this place, they must have* been carried up at least 900 feet. Mr. Maclaren‖ has described (1839) numerous blocks of sandstone on the higher parts of Arthur's Seat, "400 feet above any spot where sandstone now exists *in situ*." Quite recently Mr. D. Milne¶ has noticed other boulders on the same hill, belonging to the coal series, and remarks "that there is no place in the neighbourhood from which these blocks could have come which is not at least 200 feet below their level." In the Isle of Man, the Rev. J. G. Cumming has observed with great care a

* Journal of the Geol. Soc. vol. iv. p. 98.
† Transactions of the Geol. Soc. vol. iii. (second series) p. 13.
‡ Treatise on Geology (Lardner's Encyclop.), by John Phillips, vol. i. p. 270.
§ Philosophical Transactions, 1839, p. 69.
‖ Geology of Fife, &c. p. 47.
¶ Edinburgh New Philosophical Journal, vol. xlii. p. 167.

striking case, and has most kindly communicated to me the details, which will immediately appear in his work*: near South Barrule there is a hillock formed of granite, quite different in nature from any other rock in the island; this mass of granite is about three-fourths of a mile square, and is 757 feet above the level of the sea; from this point the boulders are thickly dispersed to the south-west, and they can be continuously followed up to a height of 788 feet above the summit of the present boss. Mr. R. Mallet informs me that facts of a similar nature have been observed in Ireland. More striking cases occur in the United States, in New England, in New York, and in northern Pennsylvania. Prof. Hitchcock observes†, that the Silurian rocks of New York and the quartz in the valleys of western Massachusetts have undoubtedly been carried over and left upon the Hoosac and Taconic mountains, at a height of "upwards of one thousand or two thousand feet." Lastly, I may mention the analogous case of the chalk-flints, associated with boulders of various kinds, observed by the Dean of Westminster and myself on Moel Tryfan, at the height of 1392 feet above the level of the sea, and which (as well as the chalk-flints at the intermediate point of the Isle of Man‡) there is good reason to believe must have come from Ireland, and therefore, at least in the case of North Wales, from a considerably lower level.

The first point to consider is whether, in these several instances, the boulders have really come from a lower level, or whether they may not (and I am indebted to Sir H. De la Beche for this caution) have been derived from strata now entirely denuded, but which formerly extended up to the same level with the boulders. Or secondly, whether the boulders, after having been deposited, may not have been raised by an unequal elevatory movement above their parent district, or the district itself have been depressed by subsidence below them. With respect to the former supposed greater extension and subsequent denudation of the parent rock,—in such cases as those near Edinburgh it is possible that this may be sufficient to account for the phænomenon. Where the boulders are of granite, as at Glen Roy and the Isle of Man, this view implies that a mass of that rock has been worn down, equalling in thickness the difference in level between the existing mass *in situ* and the boulders: in North America, where the boulders lie from 1500 to 2000 feet above their source, the denudation on this view must have been immense, and it must all have been effected within the glacial period, as the low country is covered with boulders; this likewise is the case with the boss of granite in the Isle of Man. Can it be supposed with any probability that the chalk-formation formerly extended in Ireland up to a height of nearly 1400 feet? In the case of the boulders described by Prof. Phillips, I am assured by him that the above view is quite inadmissible; and he has pointed out to me conclusive reasons, but which, considering

* The Isle of Man, its History, &c., by the Rev. J. G. Cumming.

† Geology of Massachusetts, vol. i. (Postscript, p. 5*a*), and Address to Association of American Geologists, 1841.

‡ The Rev. J. G. Cumming in Transactions of British Association, 1845, p. 61.

his high authority, I do not consider it necessary to give in detail; I will only mention that the grauwacke was planed down level, before the thick mass of mountain-limestone on the surface of which the boulders lie was deposited on it, and that at a short distance the grauwacke is quite cut off by the Craven fault: the conglomerate beds, whence the boulders at a height of 900 feet on Stainmoor were derived, are horizontal.

With respect to subsequent unequal elevation having caused the boulders now to lie above their parent rocks, the simple fact of the number of points, irregularly placed both in Great Britain (namely, in northern and central Scotland, in the Lake district, North Wales, Isle of Man and Ireland,) and likewise in the United States, appears to me to render this view extremely improbable; for on such a view it must be admitted that in Great Britain and America several great mountains and mountain-chains have been formed so lately as during the glacial period, and this is a proposition to which few geologists will be inclined to assent. Moreover, in the case of Stainmoor, it is known that its crest now holds, one part with another, the same relative level as it did during the glacial period, for the boulders have crossed it only in one notch or gap, which is now the lowest part; and certain chains of hills which would at present intercept boulders coming from one quarter likewise did so at the glacial epoch.

In the Isle of Man the parent granite and the boulders which lie 788 feet above it are scarcely more than two miles apart, and in the intermediate tract, thickly covered with the boulders, Mr. Cumming has in vain searched for evidence of a fault. In the Lake district there is, I think, conclusive evidence that unequal elevation is not the true explanation, for the boulders there lie so close to the rocks *in situ* that there would necessarily be, if the boulders had been subsequently upraised, a fault or abrupt flexure, in one case of 900 and in the other of 500 feet. Hence we must conclude, in accordance with the views of the several authors who have described the above cases, that the boulders have really been drifted nearly as many feet upwards (that is, making in almost every instance some allowance for the subsequent denudation of the parent rock) as they now lie above their original source.

Those who believe in the powerful agency of ice in moving boulders will probably at first conclude that icebergs have in some manner transported them from a lower to a higher level. But the most obvious method by which fragments of rock can get on icebergs is by their having first fallen from the surrounding precipices on glaciers entering the sea, and therefore they must have come from a higher to a lower level. It seems impossible, owing to the temperature of the water, that at any considerable depth, boulders could be frozen into the bottom of icebergs; and even if at lesser depths they did become so frozen* or mechanically wedged in, and if by the icebergs being overturned they were saved from being soon thawed out, yet they could be deposited above their former level only by so much as

* See some excellent remarks on this subject in Sir H. De la Beche's Anniversary Address for 1848, p. 68 *et seq.*

the ice under water had decreased in thickness in the interval of the boulders having been caught up and dropped. In any case the notion of icebergs having, in the States of New York, New England, and northern Pennsylvania, lifted up numerous boulders from a depth of between 1000 and 2000 feet, is quite inadmissible.

In my paper on the Boulders of the Southern hemisphere, read before this Society*, I pointed out that there were two methods, essentially distinct both in the requisite climate and in the results produced, by which fragments of rock are transported; namely, by icebergs and by coast-ice. Icebergs now transport fragments of rock on the west coast of South America, in the latitude of the central parts of Europe, under a temperate climate where the sea, even in protected bays, is never frozen. On the other hand, in the northern parts of the United States and in the Gulf of Bothnia, where the climate is excessive, but yet under a latitude where glaciers never descend to the level of the sea, fragments of rock are annually enclosed by the freezing of the coast-water, and are thus transported. In the polar regions both actions concur. Icebergs will transport such fragments of rock as fall on the parent glaciers, and these are generally quite angular. From the vast size of the bergs, the fragments will often be transported to great distances, and when deposited, it must be in deep water, and therefore (as well as from the original descending movement of the glacier) at a much lower level than the parent rock: when once dropped, they will probably never again be moved by ice. On the other hand, coast-ice will transport whatever fragments of rock or pebbles lie on or near the shore. These fragments, from being repeatedly caught in the ice and stranded with violence, and from being every summer exposed to common littoral action, will generally be much worn; and from being driven over rocky shoals, probably often scored. From the ice not being thick, they will, if not drifted out to sea, be landed in shallow places, and from the packing of the ice be sometimes driven high up the beach, or even left perched on ledges of rock. By this agency boulders will probably not be carried to such great distances as by icebergs, and the limit of their transportal will perhaps be more defined. In South America there is a considerable difference in the state of the boulders in Tierra del Fuego, where a large proportion are much rounded, and on the plains of Patagonia and in Chiloe further from the pole, where the boulders are larger and quite angular. I attributed the presence of these latter to the exclusive action of icebergs; whilst in Tierra del Fuego coast-ice appears formerly to have come into play. On Moel Tryfan† the well-rounded fragments of chalk-flints were in all probability transported by coast-ice: though I cannot doubt, from the extraordinary manner in which the laminæ of the slate rocks have there been shattered, that icebergs have likewise been driven against them when under water; so that both actions seem there to have concurred. Some other distinguishing characters between the action of coast-ice and of icebergs will presently be pointed

* Transactions of the Geol. Soc. vol. vi. (second series) p. 430.

† *London Philosophical Journal*, vol. xxi. p. 186.

out; and it is by coast-ice, as I believe, that boulders have been transported from a lower to a higher level.

To take the case of North America: Mr. Lyell* has shown, from an independent train of reasoning, that this country during the glacial period slowly subsided to a considerable amount: several American geologists have come to a similar conclusion, and they believe that the subsidence amounted to two or three thousand feet, or even more. Let us suppose a sinking movement to be now going on in the estuary of the St. Lawrence or on the coast of Labrador, where we know, from the observations of Lieut. Brown and Capt. Bayfield, as given by Mr. Lyell† (and illustrated by striking sketches), that annually an enormous number of boulders, both on and near the coast, are frozen into the coast-ice and transported to shorter or greater distances; can we doubt, that if during the year the land sunk a few inches or feet, the boulders, whilst actually frozen in or when refrozen during the ensuing winter, would be lifted up and landed so many inches or feet higher up on the coast? Capt. Bayfield, as stated by Mr. Lyell‡, saw masses of rock, "carried by ice through the straits of Belle Isle, between Newfoundland and the continent, which he conceives may have travelled in the course of years from Baffin's Bay." Now if during this probably long course of years,—for the boulders seem generally to be transported only a short distance each winter,—the land had subsided one or two hundred feet, is it not almost certain that they would have been landed so many feet higher up with respect to their former level, in the same manner as would have happened with so much drift timber? It is indeed paradoxical thus to speak of the boulders having been carried up, whilst the land has gone down; for, in fact, the boulders are merely kept by the floating ice at the same level, whilst the land sinks.

No doubt during this process some boulders would be dropped in water too deep to allow of their being refrozen, and they would be thus left behind. Scarcely any form of land would prevent the boulders from being annually landed on a temporary resting-place: even a line of perpendicular cliff, if not of very great length, would probably only cause the tidal currents to drift the coast-ice further onwards; a few more boulders, perhaps, being dropped there than elsewhere. I can see only one difficulty of any weight to this view, namely, that the boulders would be ground down into mud and destroyed from having been stranded such innumerable times, as must have happened with those which were kept up to the same absolute level during a sinking of the land of many hundred feet. On an exposed coast, where the breakers had power to dash pebbles against the boulders, I have no doubt that this would take place, more especially with boulders small enough to be themselves rolled over. But on a broken coast, amongst islands and in bays, I do not believe that this would happen. We may infer from the fact of scored rocks having been observed both in Scotland and in North Wales, dipping

* Travels in North America, vol. i. p. 99, and vol. ii. p. 48.
† Principles of Geology, 7th edit. p. 222. ‡ Ibid. p. 231.

under the surface of lakes, in a quite unaltered condition, that the action of simple water, and of such little waves as lakes can produce, even when prolonged from the glacial period to the present day, is absolutely as nothing; and in sheltered bays, the force of the waves is not very much greater than in lakes. Moreover, in South America I have seen many boulders lying on sea-beaches, exposed to the wash of rather open channels, and which, so far from having been destroyed, yet retained their angles perfect.

Nevertheless it might certainly be expected that boulders which had thus been buoyed up by coast-ice during long-continued ages would be well-rounded. According to Prof. H. D. Rogers, this is the case with the majority of the boulders in North America: those at Glen Roy were rounded, but they were composed of granite subject to disintegration; this likewise is the case with those in the Isle of Man: Mr. Cumming however informs me that the boulders, with some marked exceptions, "diminish in number and size the further we proceed" from the granitic boss. The boulders on Arthur's Seat, judging from the remarks of Messrs. Maclaren and Milne, are rounded. Those near Kirby Lonsdale, which now lie, according to Prof. Phillips, 500 feet above their parent rock, are not rounded; but they are composed of slate, a rock very little liable to be rounded, and they appear to lie in a sort of train up a valley surrounded by mountains, which must formerly have been a well-protected bay. It would be interesting to ascertain whether those boulders which now stand highest above the parent rock are more worn than those at a lower level, which latter I believe to have been dropped during the long-continued buoying-up process.

We have seen that, according to Mr. Lyell, the northern parts of the United States did actually subside during the glacial period. I am not aware that anyone has attempted to show that Great Britain was similarly affected during this same period. The following considerations, however, appear to me to render it in some degree probable: in Staffordshire there are many great and perfectly angular boulders of northern rocks, which almost every geologist believes were transported on icebergs, now lying at the height of above 800 feet above the sea; and on Moel Tryfan, at a height of nearly 1400 feet, there are stratified beds of the glacial epoch (as known by the included shells discovered by Mr. Trimmer), which beds, after careful examination, I cannot doubt were deposited in the ordinary manner under the sea. On the other hand, the character of the miocene formations, on the east coast of England, belonging to an epoch just antecedent to the glacial, lead to the conclusion that the land then did not hold a level widely different from the present one: if so, unless we suppose a great inequality in the changes of level between the east and west coasts of England, the land must have sunk after the miocene age to allow of the deposition of the glacial deposits at the heights above specified. This conclusion accords perfectly with Professor E. Forbes's statement*, that all the organic remains seen by him, from the glacial formation, indicate a depth of less than 25

* Memoirs of the Geological Survey, p. 376.

fathoms. As far then as these considerations can be at all trusted, we are, according to the view given in this paper, in a position to explain the transportal of the boulders from a lower to a higher level, in Great Britain as well as in the United States. I will make only one other remark on this head: though I believe that Great Britain subsided during the glacial period, yet I conceive it must also have subsequently attained during this same prolonged period a considerable portion of its present height. I infer this from the plain marks of true glacier action, low down the valleys in North Wales, within 300 feet of the present level of the sea*.

A second objection of apparently considerable weight has been advanced against the theory of floating ice; namely, that in some instances the blocks decrease very regularly in size in proceeding from their source. Prof. H. D. Rogers† says that this is markedly the case in going southward in the United States. According to Mr. Hopkins‡ it is also the case in the Lake district; "the blocks becoming smaller as we approach the coast of Yorkshire, till they degenerate into pebbles in the more remote localities, in which the Cumbrian rocks can be identified." He adds, "These facts are strongly in favour of those views which would refer the transport of these masses to diluvial currents." This sorting of the boulders does not always hold good: on the plains of Patagonia the two largest boulders which I saw were near the outskirts of the deposit. Sir R. Murchison also remarks on the vast size of the many boulders in the south-east parts of Shropshire, near the southern limit of his northern drift, though he elsewhere states that the boulders generally decrease in size in going from north to south. In these cases, if we look at the boulders as having all been transported on icebergs, there certainly appears no reason why they should have been dropped from such immense masses of ice, with any approach to order according to their size and to their distance from their source. But this does not hold good with boulders transported in sheets and fragments of coast-ice: here the buoying agent is not of disproportionate power to its burthen; as the ice decays, the heaviest fragments would naturally be apt to drop out first; and it would appear from the accounts given to us, that the largest boulders during some winters escape being moved at all, whilst the smaller ones are drifting onwards. Moreover, the boulders (and great stress may probably be laid on this point) which had travelled furthest, would, from having been repeatedly stranded, and necessarily so every summer, be most worn, and therefore would be smaller than those which had travelled to a shorter distance.

I have shown, in my volume on South America, that the sea has

* Since the above was written, I have found that Mr. Trimmer, in his interesting paper on the Geology of Norfolk (Journal of the Royal Agricultural Society, vol. vii. part 2), has shown that that district subsided at least 600 feet, and was likewise upraised during the boulder or glacial period.

† Address to the Association of American Geologists, 1844, p. 45.

‡ Journal of the Geological Society, vol. iv. p. 98.

the power by some means of sorting the pebbles which lie at the bottom, their size decreasing with surprising regularity, even till they pass into sand, with the increasing depth. There is some difficulty in understanding how this is effected: Playfair has suggested that the undulations of the sea propagated downwards from the surface, *tend* to lift up and down the pebbles at the bottom, and that such are liable, when thus quite or partially raised, to be moved onwards even by a very weak current. Should, therefore, a boulder formation be exposed during subsequent changes of level to the action of the sea, pebbles derived from it, and decreasing in size with perfect regularity according to their distance from their source, might be thus spread out. Hence I conceive that from a group of mountains, which had once existed as an island, boulders, decreasing in bulk with some degree of regularity, and beyond them pebbles degenerating with perfect regularity into sand, might be spread out, thus simulating the effects of a great debacle, which in rushing along had insensibly lost its power, and yet that both boulders and pebbles had been transported by the ordinary currents of the sea; aided, in the one case, by floating coast-ice, in the other, apparently by the undulatory movement of the water.

The two objections, therefore, which have been here discussed, cannot, I think, any longer be considered as absolutely fatal to the theory of floating ice; and thus far the hypothesis of a debacle is no longer necessary.

If the explanation here given of the transportal of boulders from a lower to a higher level be hereafter proved correct, we gain, in all cases where the horizontal distance between the boulders and the parent rock is not so great as to allow of the probability of subsequent unequal movements of elevation, a valuable measure of subsidence during a defined period. We are accustomed to precise measurements of elevation, from the ascertained heights of upraised marine remains; but it seemed quite hopeless to expect this, even in a lesser degree, with respect to subsidence,—that movement which hides under the sea the surface affected. It is marvellous that Nature should have thus marked by buoys made of stone, the former sinking of the earth's crust, and likewise, I may add, its subsequent elevation; and that on these blocks of stone the temperature, during the long period of their transportal, may be said to be plainly engraved. Moreover, it is thus shown that the subsidence during no one entire summer was so great as to carry the coast-boulders beneath that small limit of depth at which the salt water during each ensuing winter became frozen.

Note.—After this paper was read, Mr. Nicol objected that when the parent rock was once submerged, no further supply of boulders could be derived from it, and consequently if afterwards, each time they were afloat, only one boulder out of a hundred was dropped in water too deep for it to be refrozen in the coast-ice, after a certain time there would be none left to be carried up, during the continued subsidence, to the higher levels. This appears to me an objection of much force. I would, however, remark in the first place, that I do

not suppose that the boulders over the whole area of subsidence are carried far up, but only those in certain favourable situations. Secondly, several Arctic voyagers have stated that the pack-ice frequently piles up and leaves masses of boulders at a height of even 20 and 30 feet above high-water mark; now after a subsidence, the ice during the first gale would drive these boulders still higher up, and so onwards and upwards, with scarcely any tendency to carry them out to sea. In a bay open to the prevailing winds, and without any river entering it, I should imagine that the coast-ice would rarely be drifted outwards. Thirdly, I believe that any floating object thrown into the water not far from an extensive coast-line, is generally driven *soon* on shore: this certainly seems to be the case with the wrecks of boats; and if so, any ice-borne boulders, carried by the wind off the land, would generally be again thrown on the coast.

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3. *On Scratched Boulders.* Part I. By JAMES SMITH, Esq. of Jordan Hill, F.R.S.L.&E., F.G.S.

[The publication of this paper is postponed in order that it may appear in connexion with the second part read at a subsequent meeting of the Society.]